by me in a former communication. When exhausted by the air-pump and sealed, it showed a pressure indicated by about 0·5 inch difference in the level of the mercury; the potash was then heated; the mercury gradually fell, until it became perfectly level.

Dr. Andrews (Phil. Mag. February 1852) has shown, that with a concentrated solution of caustic potassa, he obtained with carbonic acid a vacuum with the air-pump so perfect as to exercise no appreciable tension, as no difference in the level of the mercury in the siphon-gauge could be detected.

On trying the discharge in the vacuum-tube after the potash had cooled, I found it gave the cloud-like stratifications, with a slight reddish tinge; consequently not only was the vacuum not perfect, as denoted by the form of stratification, but in this tube the colour denotes that even a trace of air remains,—probably that portion in the narrow part of the siphon-gauge, which, from its position, was not displaced by the carbonic acid.

The potash was subsequently heated until the discharge was reduced to a wave-line, with very narrow striæ; in this state moisture is seen adhering to the sides of the tube; but even in this state the difference in the level of the mercury in the gauge did not ever vary more than ·05 inch. As the potash cooled, the discharge altered through all the well-known phases of the striæ, the mercury again becoming quite level.

At first almost the slightest heat applied to the potash alters the form of the stratifications; as the heating is repeated, longer application is necessary; but it shows how sensibly the electrical discharge denotes the perfection of a vacuum, which cannot be detected by the ordinary method of mercurial siphon-gauge.

January 26, 1860.

Sir BENJAMIN C. BRODIE, Bart., President, in the Chair.

The following communications were read:—

1. "On the alteration of the Pitch of Sound by conduction through different Media." By SYDNEY RINGER, Esq., late Physicians' Assistant at University College Hospital. Communicated by Dr. GARROD. Received November 25, 1859.

Having observed that the pitch of cardiac murmurs underwent various alterations dependent on the constitution of the conducting body, the following experiments were devised to extend and render more certain the observations made on the human subject. In most of these experiments a tuning-fork was used, and in all the alterations in pitch were tested by the ear.

In making these experiments, the note of the vibrating fork was first taken with the instrument close to the ear and without being in contact with any resounding body. It was next placed on the body which was experimented on, and lastly listened to through the medium of the same. The alteration in pitch obtained by these two latter methods gave always the same results in kind, but not in degree, the alteration being always greater when the note was heard through the medium of the conducting body.

SOLIDS.

A board 13 feet long was balanced on the back of two chairs. The note of the fork was then taken, without its being in contact with the board. The fork in vibration being next placed at one end of the board, the ear was placed on the other, and the note was then found to be most appreciably lowered in pitch.

As boards composed of various kinds of wood were not obtainable, tables were used. Of all the woods thus tested, deal lowered the pitch most; indeed the lowering of the pitch was always in proportion to the porosity of the wood †.

The pitch was found to fall the greater the distance from the fork.

*Bone lowered it.

Glass raised the pitch.

Iron raised it.

† Dr. Wylde, the Conductor of the Philharmonic Society, kindly examined and fully corroborated, in those experiments marked with an asterisk, the conclusions I had previously come to. All the experiments were confirmed by numerous persons of acute ear. In no case was their opinion at variance with my own.

The two last, in conducting the note, greatly lessened the intensity, much more so than the substances described above.

The muscular substance of the heart lowered the pitch. Skin and cellular tissue, on the contrary, raised it.

Liquids.

A large foot-pan was filled with water, and the vibrating fork was partly introduced into this, but as no sound could be heard in this manner without some resounding body, a small circular piece of wood was used for this purpose; the fork placed on this was first listened to; the fork with the piece of wood was then placed under the water; one ear was then immersed in the water, and the note so taken; the pitch was then found to be most decidedly heightened. Any objections to this method of performing the experiment were obviated by the following extension of it. The eyes being firmly shut, any variation in the position of the fork, that is to say, whether it was moved closer to or further from the ear, was accurately determined by the alteration in the pitch.

Next, a glass tube 29 inches long, with a diameter of $\frac{3}{4}$ths of an inch, was closed at one end, with a diaphragm of gutta percha, oil-silk, or bladder (the same diaphragm being used in each set of comparative experiments). The tube was then filled with the fluid to be examined. The ear being applied to the diaphragm, the stem of the vibrating fork was introduced above into the fluid, care being taken that neither the finger nor the fork was in contact with the glass. Experiments conducted in the above manner gave the following results.

*Water raised the pitch most appreciably.

*Alcohol still higher.

Ether higher.

A solution of protocarbonate of soda of the same specific gravity as the blood, raised the pitch more than pure water.

* A saturated solution raised it still higher.

* Sulphate of baryta, suspended in water, raised it higher than any other tried fluid.

Prussian blue, suspended in water, raised it more than water, but less than the sulphate of baryta.

From the above results it appears that simple fluids heighten the pitch in proportion to their diminished specific gravity, and that the

addition of any substance (though increasing the specific gravity), whether in solution, or merely suspended in the water, heightens it; that particles in suspension, indeed, heighten it more than solutions.

The fact of different fluids raising the pitch in variable degrees, excludes the possibility of the rise being due to the glass, or any other material used, unless the fluid varying in weight altered the pitch, by affecting the tension of the diaphragm; but the fact of the alteration in pitch bearing no relation to the specific gravity of the fluid excludes this source of error.

The following experiments were devised to test the influence of running water on the pitch.

Into an india-rubber tube, 13 inches long, and $\frac{3}{4}$ths of an inch diameter, a funnel was inserted; immediately below this a small opening was made, just large enough to admit the end of the fork. Water was kept constantly running through this, and the stethoscope (covered with a diaphragm) applied to different parts of the tube; by this method the pitch was found to be most appreciably raised the further from the fork the stethoscope was applied to the tube. The elevation of pitch was easily recognized at a distance of $2\frac{1}{2}$ inches (the length of the pulmonary artery and adjoining part of the aorta).

The stethoscope having been unfortunately left behind, Dr. Wylde could only apply the ear directly to the tube, and therefore could not speak so decidedly as he did concerning the other experiments, but he was of opinion that the pitch was raised as stated above.

It was next attempted to be ascertained whether the mere motion of the water increased or diminished the rise of the pitch. It appeared that the pitch was very slightly raised by the mere motion of the fluid, the same point of the tube being listened to. The difference in *intensity* was most marked.

The chief object of these experiments being to ascertain the influence of the different constituents of the human body on the pitch of cardiac and other murmurs, and in order that the experiments might, as closely as possible, simulate the actual phenomena in the body, an aorta was tied to the mouth of a tap, and an artificial murmur produced by causing a constriction of the vessel by a piece of twine tied round it. The pitch of the murmur so produced was decidedly raised the further it was heard along the vessel from the point where the sound was generated.

To set the question quite at rest of the possibility of the blood in a vessel raising the pitch, especially at so short a distance as $2\frac{1}{2}$ inches, the following experiment was devised:—A tourniquet was placed over a man's femoral artery, immediately below Poupart's ligament, and an artificial murmur thus produced; this was found to rise rapidly in pitch in passing down the course of the vessel. A well-marked difference was noticed at a distance of only an inch, and decidedly more at a distance of $2\frac{1}{2}$ inches.

The *intensity* of the murmur quickly diminished in passing to the right or left of the vessel, the *pitch* being at the same time rapidly raised, which was due to the interposition of integuments; but this interposition could not be the cause of the rise of the pitch in the course of the vessel, as the murmur could be heard in that direction at a distance of at least 6 inches, whilst it was completely lost at less distance than 2 inches to either side of the vessel; thus the murmur must have been conducted by the blood, whilst the same thickness of integuments was over the artery at the lower and the upper point listened to, for both points were above the place where the sartorius muscle crosses the vessel.

Gases.

If a watch is pressed close to the ear and then gradually moved away, the tick is heard to rise in pitch in proportion to the distance the watch is withdrawn.

*Or, if in place of the watch a tuning-fork be used, the same can be still more distinctly ascertained. Then let the fork, either freely vibrating, or, still better, placed on a resounding board, be moved gradually away from the ear, the pitch will be found to rise the further the fork is carried away from the ear.

An echo of a musical note is higher pitched than the original note. Again, a loud cardiac murmur audible over the entire chest was examined in the following manner:—

The patient was directed first to exspire to the utmost, and the pitch of the heart-sound was then ascertained; he was then ordered to inspire to the full; the pitch was then found to be raised. In this experiment, the only variation was an increased amount of air between the point where the murmur was generated, and the ear of the observer.

The substances which lowered the pitch in the above experiments have one common property, namely, porosity, and, as far it could be ascertained, the depression of pitch was in proportion to this condition. Is it possible that the small vacuities included in the substance, acting as resounding cavities, and reflecting the vibrations from their walls, may so direct them that they may somewhat interfere with one another, and thereby be somewhat diminished in number? The following experiment tends in some degree to support this conjecture. It is well known that if the vibrating fork be held obliquely, resting on the table, "a loud resonance is audible; but if the tuning-fork be moved parallel to itself along the surface of the table, the resonance of the table immediately ceases from the interference of the planes of vibration with each other;" but if the fork is moved so slowly, and so that the resonance is not completely destroyed, the pitch falls slightly.

Again, if the fork be applied to the head, and listened to first with the ear open, and afterwards with the ear closed, the pitch is found to be slightly lowered.

In all those experiments in which the pitch was elevated by conduction, it was found that there was diminished *intensity* in proportion to the elevation of *pitch*; thus it would appear that all bodies raise the pitch in proportion to the difficulty with which they receive and conduct vibrations.

Dr. Scott Alison has proved in some recent experiments, that the conductivity of media, as regards rapidity, does not correspond with that of intensity. Of all tried substances, iron was the worst conductor as regards intensity, and this was found to raise the pitch most.

The above explanation is rendered somewhat probable from the fact that in all cases the elevation was greater with a weak note than a strong one. Dr. Wylde tells me that it has long been noticed by musicians that a weak note is somewhat higher pitched than a strong one, it being under these circumstances caught through the medium of the air. Those bodies which, on the contrary, lower the pitch, do so to a greater extent with a weak note.

These explanations are offered with the utmost diffidence, on account of my very limited knowledge of acoustics.

On looking into the literature of the subject, the only reference to

alteration of pitch by conduction is that by Dr. Walshe*, who ascribes it to transmission of the vibrations through "varying" media.

II. "On the frequent occurrence of Phosphate of Lime, in the crystalline form, in Human Urine, and on its pathological importance." By ARTHUR HILL HASSALL, M.D. Lond. Communicated by Dr. SHARPEY, Sec. R.S. Received November 9, 1859.

In 1854 I submitted to the Royal Society a paper "On the frequent occurrence of Indigo in Human Urine." This communication, which was published in the 'Philosophical Transactions,' attracted considerable attention both at home and abroad. The singular fact of the frequent presence of indigo in the urine, first announced by me, has since been amply confirmed by a variety of observers. I have now to place before the Society some investigations in relation to the not uncommon occurrence in human urine of *phosphate of lime*, as a *deposit*, in a well-marked *crystalline* form.

When the earthy phosphates are treated of by writers, in connexion with the urine, they are usually described collectively, and it is seldom that each kind of phosphate is particularized, and yet there are several which may occur either separately or together. The phosphate of ammonia and magnesia, or triple phosphate, is indeed often specified, but rarely is phosphate of lime separately mentioned, and phosphate of magnesia scarcely ever; and yet phosphate of lime is very frequently present as a deposit in urine, much more so, indeed, according to my experience, than the triple phosphate, excluding those cases of the occurrence of that ammoniacal phosphate, arising from the decomposition of the urea of the urine subsequent to its escape from the kidneys. Even in those few cases in which phosphate of lime is specially mentioned, it is described *usually* as mixed up with the other phosphates, and *always* as occurring in the *amorphous* or *granular*, and never in the crystalline state; further, no peculiar importance is attached to it, as contrasted with the magnesian phosphate.

* Disease of the Lungs, Heart, and Aorta. 2nd edition, page 151.